A Hierarchical Model to Evaluate Policies for Reducing Vehicle Speed in Major American Cities

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Summary of Analysis

American cities spend billions of dollars deploying traffic safety countermeasures that reduce vehicle speeds and prevent pedestrian fatalities. However, the naïve before-after comparisons commonly used to evaluate the success of these countermeasures often suffer from selection bias. In this paper, we motivate why selection bias can cause policymakers to significantly overestimate the benefits of policy using New York City's Vision Zero program as an example. We demonstrate how the NASS General Estimates System (GES) and other traffic safety databases can be combined with a hierarchical model to produce a more realistic picture of traffic safety policy. Finally, we use the results of this demonstration as evidence that New York City's estimate is optimistic, and a more reasonable estimate of the benefits of New York City's Vision Zero policy may be two-thirds the size originally claimed.

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I. Introduction

Vision Zero Policy in Theory and Practice

Researchers cite motor vehicle collisions as a leading source of preventable death in the United States (Mokdad et al. 2004). Thirty five thousand Americans were killed by motor vehicles in 2013. This exceeds the number of fatalities from more controversial causes such as firearms and alcohol. There were 34,000 deaths from firearms injuries and twenty-nine thousand deaths induced by alcohol in 2013 (Xu et al. 2010). Yet gun and alcohol related deaths receive a significantly larger portion of the public's attention.

The difference between collisions and guns or alcohol is that policymakers have historically considered fatalities from motor vehicle collisions unavoidable. In fact, traditional road design manuals intended for highways explicitly recognized a tradeoff between safety and mobility. Engineers were instructed to build roads to accommodate the projected traffic volume and reduce bottlenecks. For example, urban arterials were designed to achieve a traffic flow of 30 to 60 mph (Aashto 2001). Policymakers then set speed limits to the 85th percentile speed of drivers observed in favorable operating conditions. This speed was considered a reasonable balance of safety and mobility, and drivers were expected to reduce their speed only when unfavorable operating conditions produced increased risk (National Research Council (US). Transportation Research Board and Limits 1998).

An increasing number of American cities have since found the traditional approach inadequate because drivers in urban areas are seldom able to identify the unfavorable operating conditions that put more vulnerable road users like pedestrians or bikers at increased risk[1]. In 2014, fifteen percent of motor vehicle fatalities were pedestrians, and seventy-eight percent of those fatalities occurred in urban areas (Administration 2016). Many cities have since established countermeasures that encourage road users to make safer decisions. The policies are collectively known as Vision Zero and have a stated goal of creating a road system with zero traffic fatalities.

Vision Zero originated as a comprehensive, long-term infrastructure investment strategy that rejects the traditional balance of safety and mobility. It prioritizes safety over mobility by anticipating human error and then slowing vehicles to the safest possible travel speed. The success of this strategy is guaranteed by physics: slower vehicles collide with less force and are therefore less likely to kill a pedestrian. Citywide road redesign that actively slows vehicles is therefore integral to the Vision Zero strategy as originally conceived (Tingvall and Haworth 2000).[2]

Although theoretically effective, road redesign has proven costly to implement and challenging to sustain across an entire city. An expedient albeit less effective countermeasure is to simply mandate that vehicles reduce their travel speed by lowering the posted speed limit below the speed for which the road was originally designed (Leaf and Preusser 1999). The posted speed limit can be reduced immediately and at relatively little cost, and the reduction ostensibly affects every vehicle across an entire city.

Needless to say a posted speed limit without commensurate levels of design, enforcement and outreach is unlikely to achieve the Vision Zero goal because drivers have little incentive to reduce their speed in compliance with the lower limit (Leaf and Preusser 1999). But making these improvements provides no guarantee. The National Highway Safety Traffic Administration rates the countermeasure "reduce and enforce speed limits" three out of five stars for improving safety because research indicates that actual speed is reduced by only a fraction of the lowered amount, typically 1-2 miles per hour (mph) for every 5 mph reduced. It states in no uncertain terms that effectiveness requires the reduced limit be met with communications

[1] Often collisions involving pedestrians occur in the middle of the road at night (Administration 2016), perhaps indicating that drivers frequently fail to notice jaywalking pedestrians. Nevertheless, the driver is found at least partially responsible in the vast majority of these collisions.

[2] Vision Zero and the traditional approach can differ substantially. For example, traditional traffic safety improvements often widen roads and crosswalks to aid evasive maneuvers that reduce collisions, while Vision Zero advocates improvements that narrow them, much like the road "diets" of Goodwin et al. (2010), to force drivers to decrease their vehicle speed. Consequently, reducing fatalities under the Vision Zero approach may actually increase the number of collisions. For details see Johansson (2009).

and outreach, enforcement and engineering changes. It warns that merely changing speed limits is of limited effectiveness (Goodwin et al. 2010).

Nevertheless, Vision Zero advocates promise American cities huge reductions in fatalities when speed limits are lowered as part of a larger Vision Zero strategy. Many cite European countries like Sweden, which pioneered the Vision Zero movement with partnerships between industry and government in the mid-1990s (Government Offices of Sweden and Investment Council, n.d.). Sweden took steps such as reducing the speed limit, separating traffic lanes and erecting barriers. Fatalities fell from 6 to 4.7 deaths per 100,000 residents (Johansson 2009), meanwhile the United States continued to experience 11 deaths per 100,000 residents in 2013.

Twelve major American cities have since set a Vision Zero strategy: Chicago, San Francisco, New York City, Boston, Los Angeles, Austin, Portland, Seattle, San Jose, San Diego, Washington D.C. and Denver. The majority of these strategies include adjustments to many or all of the posted speed limits. For example, New York City lowered the default citywide speed limit from 30 to 25 mph in late 2014. San Francisco uniformly set the speed limit around schools to 15 mph, and is considering a citywide speed limit of 20 mph at the time of this paper. The policy question is whether these reductions are nominal legislative changes or whether vehicle speed has been lowered enough to meaningfully diminish the probability a collision results in a pedestrian fatality.

Selection Effects Bias Before-After Comparisons

The Vision Zero goal of zero fatalities offers little direction on how to evaluate Vision Zero policies because any death prevents a city from achieving this goal. However, traffic safety policy has significant economic and quality of life costs to city pedestrians, and the success of Vision Zero policies to meaningfully reduce fatalities might best be evaluated within this context. For example, the U.S. Department of Transportation directs it offices to assign human life a value of around $10 million when conducting analysis that carries implications for public safety (Transportation 2016). Thus from a cost-benefit perspective, the billions of dollars invested on countermeasures by American cities annually might be considered successful if it were to save hundreds of lives. Unfortunately, even a vague estimate of the number of lives saved from traffic safety improvements is notoriously difficult to estimate as we illustrate in the present section. Hauer (2005) and Davis (2000) provide extended discussions.

Traffic safety research relies predominantly on before-after comparisons to evaluate policy in lieu of randomization, the standard of experimental research. A before-after comparison simply examines the outcome of a policy before and after its implementation and attributes any change to the policy, after possible adjustment. The key assumption is that the before period reflects the outcome one would expect had the policy never occurred. A before-after comparison can be made in various ways, and the exact formulation is generally a matter of taste. Popular with traffic safety researchers are modification factors that measure the ratio of the outcome after the policy to before. The FHWA Highway Safety Manual evaluates policy in this way. We work instead with the reduction factor, which measure the difference between the before and after outcomes. Policymakers more commonly report this measure.

We describe a typical before-after comparison using New York City's Vision Zero strategy (Taskforce 2017). This description motivates the major limitation of before-after comparisons. Throughout this section and the remainder of the paper we consider only pedestrian fatalities.

In November 2014, New York City reduced the citywide default speed limit from 30 to 25 mph. The City Department of Transportation and Police Department established a committee tasked with enforcing the speed limit in key areas. To determine these areas, the committee reviewed the location of vehicle-related fatalities and serious injuries between the years 2009 and 2013. These locations were then prioritized for Vision Zero engineering, enforcement and education changes. Using the road shapefile provided by the New York State GIS Program Office,[3] we estimate 44,337 road segments or about 32 percent of New York City

[3]https://gis.ny.gov/aboutus/

road segments were prioritized. We found these road segments made up roughly 70 percent of pedestrian fatalities between 2009-2013.[4]

The priority locations are visualized for midtown Manhattan Figure 1. The blue shaded region is the priority area, the blue lines are the priority corridors and the blue triangles are the priority intersections. Blue dots depict fatalities falling within priority locations. Red dots depict fatalities falling outside priority locations.

Fatalities are displayed in Figure 1, panel 1 for the last review year, 2013, and in panel 2 for the year 2016. These years correspond to a before-after comparison reported by the committee as evidence that Vision Zero was successful. The committee noticed that over the 2009-2013 period, an average of 99 pedestrian fatalities occurred at priority locations throughout the city each year. In 2016 there were only 72 fatalities at these locations, a 27 percent decline. They concluded this decrease was due to the changes made since 2015 (Taskforce 2017).

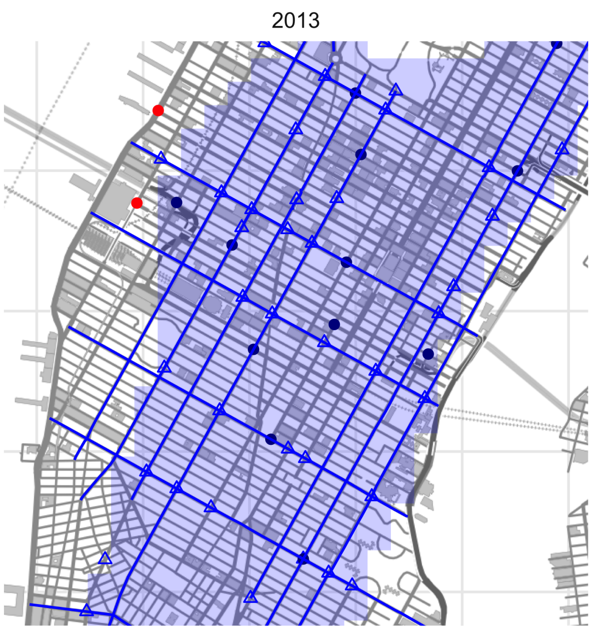


Figure 1: This figure displays the locations of pedestrian fatalities in mid-Manhattan for the last year (2013) of the before period (2009-2013) and the after period (2016). Priority corridors are colored blue, priority intersections are triangled and priority zones are shaded blue. Fatalities in priority locations are represented by blue dots, while fatalities outside of priority locations are represented by red dots.

The annual number of fatalities from 2009 to 2016 is displayed in Figure 2, and the city's conclusion is reflected in the first panel that only includes road segments in priority locations. For the present discussion, we interpret the committee's claim that Vision Zero policy caused the reduction from an average of 99 to 72 deaths as a statement regarding the average treatment effects in the potential outcomes framework of Imbens and Rubin (2015). That is, 27 deaths were averted in 2016 because of Vision Zero policies in the sense that we expect these deaths would have occurred had the before period level of traffic safety been maintained and not improved. Furthermore, we presume the claim is stating that this reduction is similar to the level of fatalities the city can expect in future years when similar roads segments in other parts of the city are treated in a similar manner.[5]

[4] Our visualizations differ from New York City's Vision Zero evaluation, possibly because of different data sources, although we notice no difference in the conclusions. Our data is from the Department of Transportation's data feed http://www.nyc.gov/html/dot/html/about/vz_datafeeds.shtml.

[5] This is not exactly equivalent to the key assumption of the before-after analysis that the before period reflect the fatality

A simple thought experiment using results from the traffic safety literature suggests this interpretation is optimistic but not impossible. Suppose for simplicity all vehicles traveled uniformly at 30 mph before the City reduced its default speed limit. The National Highway Safety Traffic Administration estimates that a 5 mph reduction of the speed limit reduces vehicle speeds by 1-2 mph. Assuming every priority road achieved the maximum reduction in vehicle speeds, a new speed limit of 25 mph would lower vehicle speeds to 28 mph. According to Rosén and Sander (2009),[6] this 2 mph change in vehicle speed would yield a fatality reduction of 24 percent, 3 percentage points lower than the decrease suggested by New York City's Vision Zero committee. The maximal attainable reduction can be seen as 50 percent, the decrease in fatalities if the city might potentially achieve were it able to successfully reduce vehicles to the posted limit of 25 mph.

The second two panels in Figure 2 indicates the extent to which the 27 percent reduction in fatalities cited by the Vision Zero committee likely overstates the success of Vision Zero policies. It shows that from the 2009-2013 before period to the 2016 after period, fatalities in non-priority locations increased by 20 percent, roughly the same magnitude as fatalities in priority locations decreased. Moreover, little change in the total number of pedestrian fatalities occurred over the rollout of New York City's Vision Zero program from 2014 to 2016 as demonstrated in the third panel of Figure 2. This striking antiparallel behavior among priority and nonpriority roads and relatively minor progress in reducing fatalities as a whole over the last few years suggests the procedure used by New York City to select priority roads, and not the Vision Zero program itself, accounts for a significant portion of the observed changes as in the following scenario.

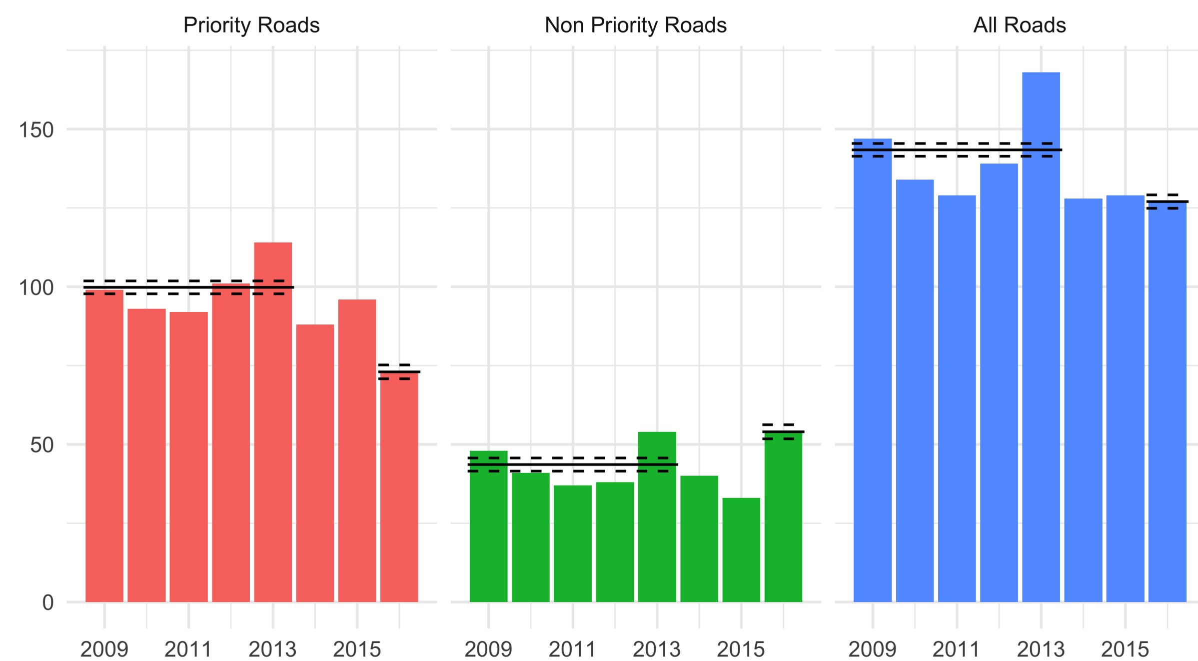


Figure 2: This figure displays the number of pedestrian fatalities in New York City by year (bars, colored by priority status). The Vision Zero: Year Three report highlights the drop in fatalities in priority roads from an average of 100 over the 2009-2013 time period to 73 in 2016 (solid lines, with broken lines representing 95% confidence intervals). However, fatalities on non-priority roads increased 25 percent.

rate had the policy never happened because we would still expect investment and depreciation of road segments. We point out that our model could be adapted for a difference-in-difference analysis, which would more accurately reflect the benefit of the policy. However, this is not the comparison made by New York City's Vision Zero committee and would move us away from the more basic before-after comparisons that are the subject of this paper and more typically relied upon by policymakers.

[6] Rosén and Sander (2009) look at the probability of a fatality following a collision on roads in Germany that may differ from roads and customs in New York City. Moreover, Vision Zero improvements may increase the number of collisions, moderating the benefits of a lower speed. Traffic safety improvements sometimes distractions which could potentially account for a lower fatality rate without reducing vehicle speed.

We know the committee chose priority roads with abnormally large numbers of fatalities. It is likely some portion of the fatality rate on the selected roads was due to dangerous conditions in need of repair and some portion was due to coincidence and unlikely to repeat itself. The fact that as many as 139 thousand street segments could have been selected as priority indicates that perhaps a sizable portion of fatalities on priority roads were coincidence. The fatalities on these roads would be unlikely to repeat themselves, and as a result, the total number of fatalities over the group of roads would revert from the abnormally high levels of the before period to the city-wide mean when measured in subsequent years, regardless of any intervening policy. A similar behavior could be ascribed to the unselected roads segments: these had abnormally low numbers of fatalities and were expected to see a rise, regardless of policy.

If true, the before measurement is higher than what we would expect in both the 2009-2013 period and in subsequent years had the same levels of traffic safety policy been maintained. This violates the key assumption that the before period reflects the outcome one would expect had the policy never occurred. As a result, the before-after comparison of priority roads may overstate the causal effect of the Vision Zero policy. The exaggeration of the estimated effect of Vision Zero policy in this manner is often referred to as selection bias since the Vision Zero committee selected roads more likely to have a subsequent reduction in fatalities.

Nonparametric Empirical Bayes Adjustment for Selection Bias

Recognition of regression effects and selection bias lie at the foundation of modern statistical practice (Stigler 2016, see pg. 139 for the cautionary tale of Horace Secrist). Moreover, traffic safety policy, along with students' test scores and parent/children heights, have long served as a prime example. It is no surprise then that many statisticians have offered corrections. Among the most famous is Herbert Robbins', who even considered the exact problem of selection bias in before-after studies (see for example, Robbins and Zhang (1988)). His Empirical Bayes approach borrows information cross-sectionally, across units researchers might normally consider incomparable, to construct more realistic expectations. Its success comes from the recognition that, while the units themselves may be different, the ways in which they vary are systematic and can thus be characterized. For a concrete example, consider the hierarchical model:

$$
\begin{aligned}
X_i &\sim \text{Poisson}(\lambda_i) \\
\lambda_i &\sim \text{g}(\theta)
\end{aligned}
$$

In the context of New York City's Vision Zero policy, X_i denotes the number of pedestrian fatalities on road segment i in any year between 2009-2013. The rate parameter of each road segment, λ_i, is thought exchangeable and modeled i.i.d. $g(\theta)$. The reader may wish to interpret this hierarchy as describing how road segments might themselves be chosen from a superpopulation of road segments that a city could build. Denoting I as the set of all priority road segments, we write the expected number of pedestrian fatalities on these road segments under the 2009-2013 policy after observing the number of fatalities from 2009-2013 as $E(\sum_{i \in I} \lambda_i | X_i) = \sum_{i \in I} E(\lambda_i | X_i)$. Each individual $E(\lambda_i | X_i)$ is estimable using Robbins' formula (Efron and Hastie 2016):

$$
\hat{E}(\lambda_i | X_i) = (X_i + 1) \frac{\sum_j X_j \mathbb{1}(X_j = X_i + 1)}{\sum_j X_j \mathbb{1}(X_j = X_i)}
$$

This estimate was originally suggested by Alan Turing as a solution to the similar Missing Species Problem of Good (1953), but named after Herbert Robbins who extensively studdied its properties (Robbins 1955, Robbins and Zhang (2000)). Its appeal comes primarily from the fact that no restrictions have been placed on the prior/population distribution, $g(\theta)$.

Table 1 applies Robbins' formula to the Vision Zero data. The first row, <1> prints the total number of road segments in New York City by the number of fatalities. For example, there were 138,142 segments with no fatalities, 632 with one fatality, and so on. These were plugged into Robbins' Formula to get <2>, the expected number of fatalities on each road segment given the number of fatalities observed. This was then multiplied by <3>, the number of road segments selected by the Vision Zero committee as priority locations, yielding <4>, the number of fatalities on priority road segments we would expect over a five year period if there were no policy changes. Summing and dividing by the number of years in the 2009-2013 period gives an average of 52 deaths. Thus despite observing an average of 99 deaths in priority locations over the 2009-2013 period, we estimate an average of 52 fatalities a year in these locations.

Table 1: Application of Robbins' Formula to the New York City Vision Zero data

Observed Number of Fatalities	0	1	2	3
<1> Number of Road Segments	138,142	632	40	1
<2> Estimated Fatality Rate	.0045	.1266	.075	4
<3> Number of Priority Segments	43,806	405	29	1
<4> Expected Number of Fatalities	200	51	2	4

This expectation of 52 fatalities per year in the priority locations is substantially lower than the 73 fatalities actually observed in 2016 after Vision Zero changes. Robbins' formula reports more fatalities in priority locations have occurred than would be expected if the previous policy had been maintained, not less, as claimed by the Vision Zero committee. Moreover, this increase is massive. It implies the percent increase in the average number of deaths from the 2009-2013 before period to the 2016 after period would be a whopping 27 percent.

Yet we believe this 27 percent increase is potentially as pessimistic as the number estimated by the New York City committee is optimistic. Nothing in the traffic safety literature indicates that any of the countermeasures employed should substantially increase the number of fatalities. More likely, we have too readily compared New York City priority road segments with nonpriority road segments when in reality the two might be quite disparate.[7] Unfortunately, it is difficult to gauge how pessimistic this adjustment actually is without additional information or imposing additional structure.

We conclude that the inclusion of prior information with an interpretable model is necessary to reliably estimate the benefits of traffic safety policy aimed at reducing vehicle speed in major American cities. Indeed, this is the objective of our analysis. We turn to hierarchical Bayes, which favores comparably with empirical Bayes, even without the inclusion of structural prior information (Berger 2013). Nevertheless, we use the vast quantity of traffic safety data available to group road segments by various types. We assume that roads segments are exchangeable within type, allowing us to easily quantify the sources of variation which contribute to the fatality rate.

Contrary to the Nonparametric Empirical Bayes analysis, we find evidence that Vision Zero policy did decrease the number of fatalities. However, there is much uncertainty surrounding the increase, and reductions ranging from negligable to the size claimed by the Vision Zero committee are plausible. Yet we believe it is reasonable to state that the decrease is roughly two-thirds the size claimed by the Vision Zero committee.

II. Selecting Predictors of Pedestrian Fatalities

The previous section motivated the need to incorporate additional information about road segments into our estimate of the fatality rate before the introduction of Vision Zero policy. We suspect qualitative differences between road segments, such as the number of lanes or the existence of a protective median, are disproportionately distributed across priority and nonpriority roads. It was unclear how such information could be integrated into Robbins' formula to yield a more reliable and interpretable estimate of the benefits

[7]It is unlikely all 44,337 road segments in New York City were eligible to be priority locations. For example, it may be unrealistic to include road segments on the periphery of the city.

of Vision Zero policy. Our contribution can best be seen as full Bayes extension of the hierarchical model from the previous section that adds qualitative details to $g(\theta)$ informative of fatalities.

Covariates that represent major sources of variation in pedestrian fatalities are without a doubt important. These covariates determined the selection procedure used by the Vision Zero committee and subsequently confound the evaluation of priority locations in the before-after comparison. But experts disagree on which variables are most important and their exact mathematical relationship with each other and the fatality rate. An interpretable model will allow us to evaluate which covariates are important and improve the model. We presume the relationships between these important covariates and fatalities will persist across years and cities, and we jointly model fatalities from the twelve major American cities that have declared Vision Zero policies in order to better understand these relationships.

Our primary dataset in this endeavor is the National Automotive Sampling System (NASS) General Estimates System (GES) collected by the National Highway Traffic Safety Administration. Multiple datasets of fatalities and covariates informative of fatalities exist across many different government entities. For example, our motivating example relied upon fatality data from New York City Department of Transportation's fatality database and road segment data from New York State's GIS Program Office. However, these datasets contain few informative covariates, and many of these covariates only cover specific regions within cities and are subject to coding standards that render them incomparable across regions. Datasets that do contain standardized information across multiple cities are largely retrospective since fatalities are rare, and it is easier to collect information after a fatality has occurred than to continuously update information about roads.

Fortunately, the GES is a nationally representative probability sample selected from the more than five million police-reported crashes that occur annually. To our knowledge it is the only prospective measure of fatality risk that contains a large number of covariates detailing the road, vehicle and persons involved. For additional details on the GES, see Roya et al. (XXXX). We combine the GES with the Fatality Analysis Reporting System, (FARS), a retrospective census of fatalities with similar covariates. The main appeal of FARS is the inclusion of identifying information, like geographic coordinates, that allow us to estimate traffic and pedestrian density and use our inferences to evaluate New York City's priority roads segments as discussed in the Introduction.

Our combination of GES and FARS has a strong connection to the multilevel regression and poststratification approach popular in the survey literature (Gelman and Little 1997), (Gelman and Hill 2006), and perhaps it can be best motivated with an analogy to survey sampling. We think of the weighted GES data as a representative sample of road segments by type, where type is determined by levels of select covariates. FARS is, in contrast, a convenience survey of road segments, which oversamples roads with covariate levels predictive of the outcome of interest: fatalities. This oversampling allows us to better understand the covariates predictive of fatalities, which we quantify a hierarchical model. In this capacity, the GES serves as a Rosetta Stone, allowing us to re-adjust retrospective information to reflect a desired population of road segments.

Re-adjustment follows directly from the Law of Total Expectation. Let X_i be the number of fatalities on the i^{th} road segment, $X_i \in \{0, \ldots, \infty\}$ and Z_i the "type" of the i^{th} road segment, $Z_i \in \{0, \ldots, M\}$. Then by the Law of Total Expectation,

$$
\begin{aligned}
\mathbb{E}(X_i|X_i \geq 1, Z_i = z)\mathbb{P}(X_i \geq 1|Z_i = z) &= \mathbb{E}(X_i|X_i \geq 1, Z_i = z)\mathbb{P}(X_i \geq 1|Z_i = z) + 0 \\
&= \mathbb{E}(X_i|X_i \geq 1, Z_i = z)\mathbb{P}(X_i \geq 1|Z_i = z) + \mathbb{E}(X_i|X_i = 0, Z_i = z)\mathbb{P}(X_i = 0|Z_i = z) \\
&= \mathbb{E}(\mathbb{E}(X_i|X_i \geq 1, Z_i = z)|X_i \geq 1) \\
&= \mathbb{E}(X_i|Z_i = z)
\end{aligned}
$$

Our goal is to estimate $\mathbb{E}(X_i|Z_i = z)$. We arrive at this quantity by estimating $\mathbb{P}(X_i \geq 1|Z_i = z)$ from the GES using the empirical probability of a fatality on each road type
$\sum_i \mathbb{1}(X_i \geq 1, Z_i = z)/\sum_i \mathbb{1}(Z_i = z)$ and
$\mathbb{E}(X_i|X_i \geq 1, Z_i = z)$ with a hierarchical model detailed in the following section. The empirical estimate of

$\mathbb{P}(X_i \geq 1|Z_i = z)$ is conditional on the event that the road segment type is included in the GES sample. Since every collision on every road segment could potentially be included in the GES sample, we use the GES stratified-sampling scheme referred to in the GES Users' Manual to adjust the probability for unconditional estimates. Reversing this weighting procedure for a different region yields the expected number of fatalities over that region. Indeed, we will use this procedure to obtain the expected number of fatalities for New York City priority road segments.

The success of this strategy depends on the whether roads segments are believably exchangeable given covariate levels, and thus the road segments within the FARS convenience sample are representative of all road segments with those covariate levels. This assumption is considered essentially satisfied if there is far more variation between covariate levels than within covariate levels. We evaluate the plausibility of this assumption with an Analysis of Variance in the following section.

We conclude this section by briefly noting several additions to the final dataset used for this analysis. Missing New York City speed limit data in FARS is imputed from NYC DOT using the shapefiles of speed limits. The pedestrian population on each road was estimated following the recommendations of the Census Bureau using the Census Tract Flows (TPP) and the Census Planning Database (PDB). Average annual daily traffic was estimated from the Highway Performance Monitoring System (HPMS).

The final dataset contains 2,404 fatalities as determined by the KABCO Scale.[8] It includes every pedestrian death in the twelve major American cities with some version of a Vision Zero policy between 2010 and 2015 for which there was no missing covariate information that could be reliably estimated with additional resources. We assume missingness results from unintentional oversight and therefore is missing at random. The year 2009 was dropped because of compatibility issues between GES and FARS.

Summary of Dataset

The dataset has the following eight qualitative variables describing each road segment: the weather and surface condition (COND), the city (CITY), the year (YEAR), the posted speed limit (SLIM), the presence of various signs or signals (SIGN), the time and lighting (LGHT), the physical road characteristics or built environment (BLTE) and the annual average traffic density (TFFC). The first six observations of the dataset are displayed in Table 2 below. These variables can be thought of as batches of covariates that characterize road segments.

Conversations with experts indicated that these batches express covariates with similar amounts of informativeness a priori. For example, CITY denotes which of the twelve cities contains the road segment, and we expect the contribution of the fatality rate due to the city containing the road segment to vary systematically about the average city level. Interactions between the categories are likely, but the same conversations suggested that their importance is second order, and a cursory analysis might exclude them entirely. The dataset also has one quantitative variable: the number of pedestrians exposed to the road segment (EXPR). Sampling weights were not used in the hierarchical model and are excluded from the discussion of the current section.

Table 3 shows the number of observations within the most common qualitative variable levels. A few of these qualitative variables possess a large number of categories. COND, LGHT and BLTE have 25, 36 and 111 categories respectively, and most of these categories are well represented. For example, New York City (city 12) had around 800 road segments and many roads are observed in weather condition 2 (clear weather, dry road surface). But while the marginal counts of each category can be large, the number of observations within interactions, or combinations of categories, quickly dwindles as the following three figures demonstrate.

Table 2: Example Observations of Dataset

COND	CITY	YEAR	SLIM	SIGN	LGHT	BLTE	TFFC	EXPR
2	1	1	6	1	10	4	1	256.52

[8] https://safety.fhwa.dot.gov/hsip/spm/conversion_tbl/pdfs/kabco_ctable_by_state.pdf

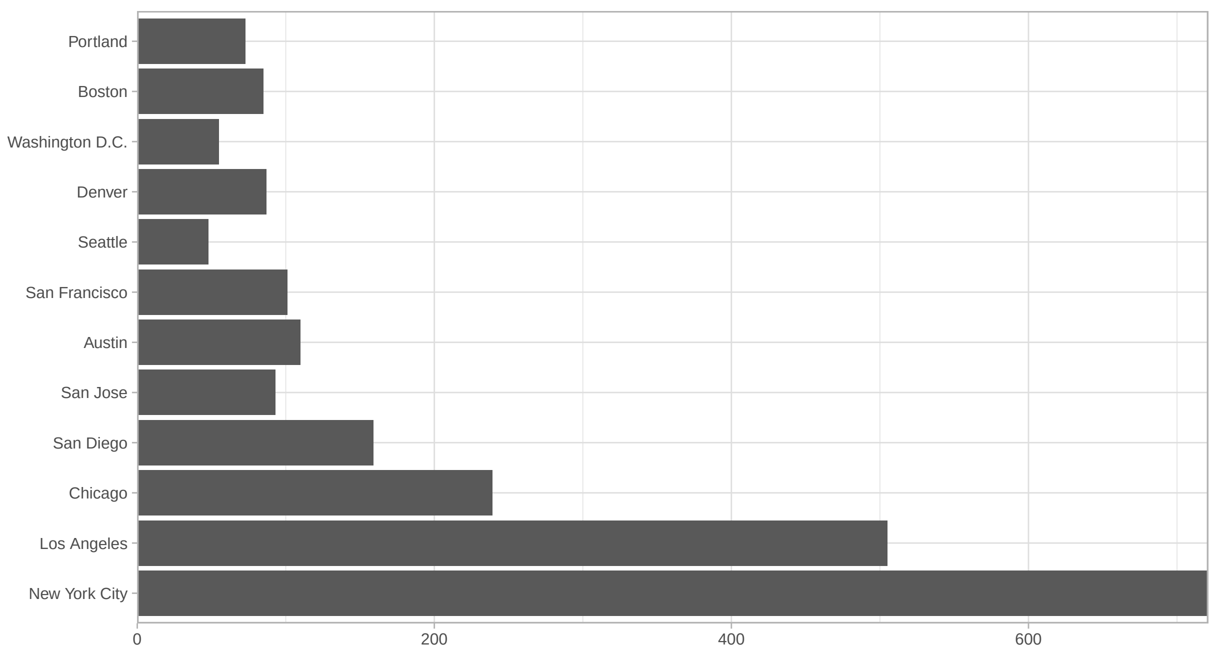
COND	CITY	YEAR	SLIM	SIGN	LGHT	BLTE	TFFC	EXPR
2	1	1	6	1	29	4	1	382.92
2	1	1	6	13	29	6	1	859.79
2	1	1	6	3	8	6	1	3117.68
2	1	1	6	1	8	9	1	3286.61
2	1	1	6	1	14	18	1	193.75

Table 3: Summary of Dataset

COND	CITY	YEAR	SLIM	SIGN	LGHT	BLTE	TFFC
2 :1708	12 :721	1:354	7 :768	1 :1423	21 :335	6 :530	1: 55
23 : 223	8 :505	2:356	8 :523	4 : 397	8 :298	18 :405	2:1332
9 : 202	6 :239	3:408	6 :351	3 : 313	34 :223	14 :147	3: 769
24 : 54	9 :159	4:406	10 :150	7 : 69	29 :217	7 :114	4: 120
3 : 27	3 :110	5:369	14 :127	10 : 23	1 :189	8 :103	NA
18 : 12	10 :101	6:383	9 :127	2 : 18	25 :135	13 : 93	NA
(Other): 50	(Other):441	NA	(Other):230	(Other): 33	(Other):879	(Other):884	NA

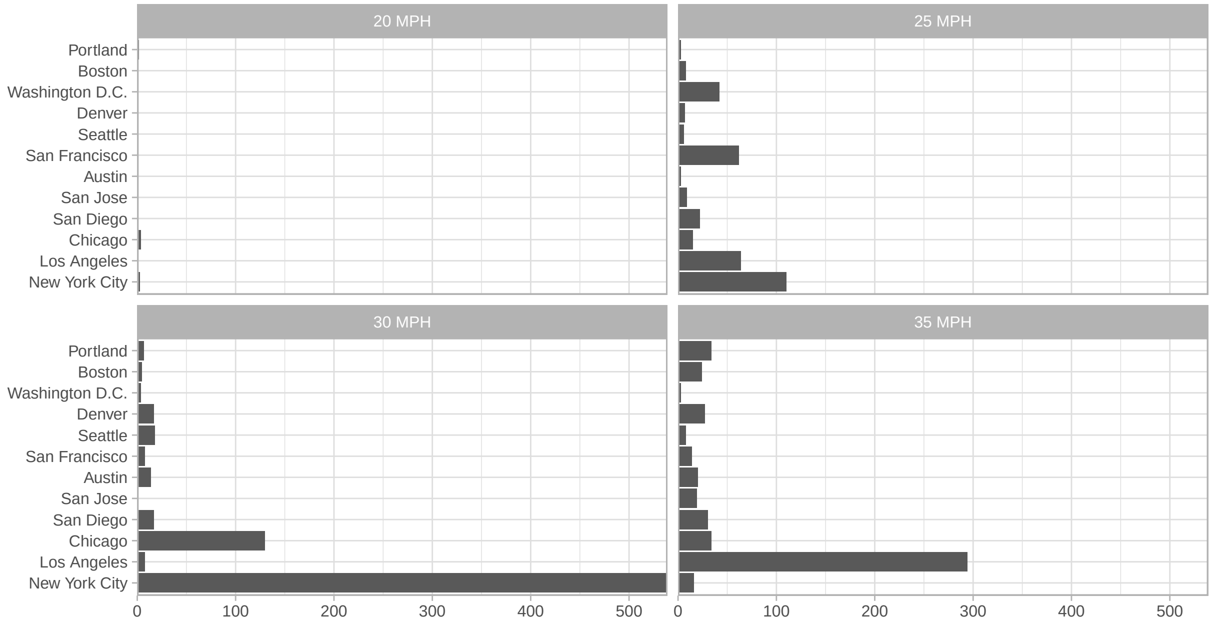
Figure 3.1 plots a histogram of the observations by city. Figure 3.2 plots these cities by the most common speed limits, and Figure 3.3 plots them by both the most common speed limit and the last four years of the dataset. Cities are arranged from smallest population at the top to largest population at the bottom (as of the 2010 Census). In the interaction between speed limit, city and year, the largest cell only has 150 observations, and the vast majority has less than 10 observations. And this does not even include the larger categories COND, LGHT and BLTE.

Figure 3.1: Number of Observations per City



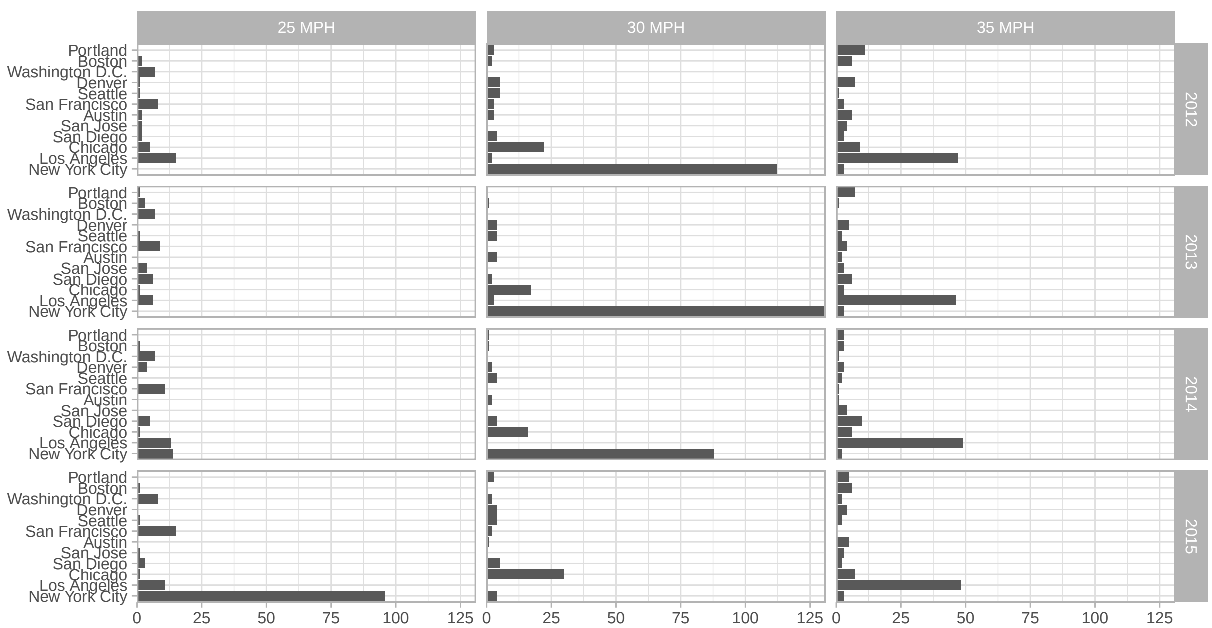
This figure exhibits the number of deaths in each Vision Zero city over the 2010 to 2015 sample period. Cities are arranged in increasing order of population. The number of fatalities corresponds loosely with population size.

Figure 3.2: Number of Observations per City by Posted Speed Limit



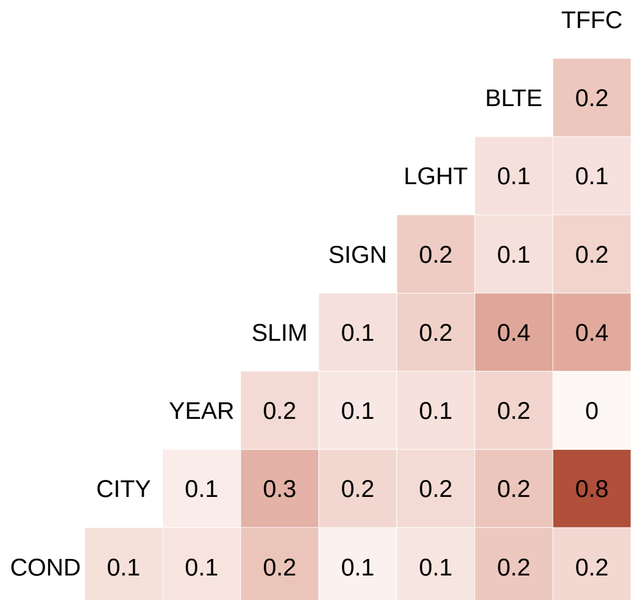
This figure exhibits the number of deaths in each Vision Zero city over the 2010 to 2015 sample period stratified by whether the posted speed limit on the road was 20, 25, 30 or 35 MPH. Cities are arranged in increasing order of population. Speed limits of 25, 30 and 35 are observed for every major city.

Figure 3.3: Number of Observations per City by Posted Speed Limit and Year



This figure exhibits the number of deaths in each Vision Zero city over the 2012 to 2015 sample period stratified by the year each fatality took place and whether the posted speed limit on the road was 25, 30 or 35 MPH. Cities are arranged in increasing order of population. Speed limits of 25, 30 and 35 are not observed for every major city in every year.

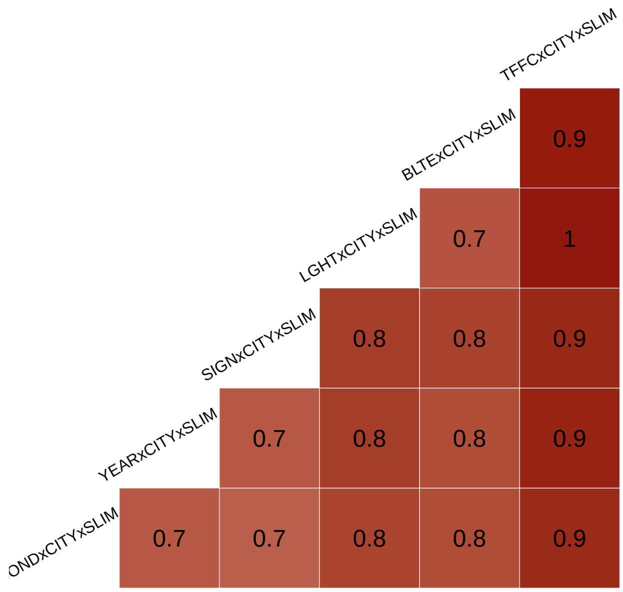
Figure 4.1: Correlation of Qualitative Variables using Cramer's V



This figure exhibits the marginal correlation of the eight qualitative variables using Cramer's V statistic. A value of 0 indicates that the two variables have little association, and a value of 1 indicates that the two variables are equal to each other. From this figure we observe that nearly all of the variables are moderately associated. The largest association exists between the city and traffic density.

Low counts within combinations of the categorical variables can lead to instability and interpretability issues if the categorical variables are highly associated. We summarize the pairwise association of the eight qualitative variables using Cramer's V statistic in Figure 4.1. We find that no two covariates, aside from year and traffic density, are highly associated although nearly every covariate exhibits moderate association. This suggests we can proceed with all eight qualitative variables in our initial model. The high associations in Figure 4.2 warns that we must be judicious in deciding among interactions between the categories because the interaction terms are highly associated with each other.

Figure 4.2: Correlation of Select Interactions using Cramer's V



This figure exhibits the marginal correlation of select interactions using Cramer's V statistic. A value of 0 indicates that the two variables have little association, and a value of 1 indicates that the two variables are equal to each other. From this figure we observe that all of the variables are moderately associated. The largest association exists between the posted speed limit, traffic density and the built environment.

III. Hierarchical Model for Fatalities in the Before Period

The data described in the previous section constitute a high dimensional contingency table where each cell is a low, non-zero count of road segments. It is conventional to employ a zero-truncated, log-linear model in this case. Let y_{ij} denote the i^{th} death in the j^{th} covariate stratum. The joint probability distribution is then factorized as follows:

$$
\begin{aligned}
\epsilon &\sim \text{Normal}(0, \sigma_\epsilon) \\
\alpha_i^{\cdot} &\sim \text{Normal}(0, \sigma_i) \\
\bar{y}_{\cdot j} &\sim \text{Poisson}^+(\exp(\mu + \alpha_1^{\text{SLIM}} + \alpha_2^{\text{CITY}} + \alpha_3^{\text{YEAR}} + \alpha_4^{\text{COND}} + \alpha_5^{\text{SIGN}} \\
&\qquad + \alpha_6^{\text{LGHT}} + \alpha_7^{\text{BLTE}} + \alpha_8^{\text{TFFC}} + \epsilon_j + \beta \cdot \log(\text{EXPR}_{\cdot j})))
\end{aligned}
$$

To complete the specification of this model (Model 1), we put diffuse[9] normal priors on μ, β and the σ. In the next section we expand this model (Model 2) with interactions between some of the explanatory variables.

We sample from the posterior distribution of the parameters, excluding year 2015 from the dataset for validation. Specifically, we use RStan (Stan Development Team 2016) to run four chains for 2,000 iterations each. This yields 4,000 posterior samples after discarding the first 1,000 warm-up iterations of each chain. In the Stan generated quantities block, we calculate the finite sample standard deviations of each qualitative variable (Gelman 2005). We also estimate the number of fatalities of each road type in the dataset in the year 2015, first supposing the region had a 25 mph posted speed limit and then supposing it had a 30 mph posted speed limit. The fact that New York City lowered its speed limit from 30 to 25 mph provides a natural experiment, and we use these samples to assess whether the proposed model can adequately extrapolate

[9] https://github.com/stan-dev/stan/wiki/Prior-Choice-Recommendations

across strata. The estimates are consistent with the data, although we do not include visualizations of these posterior checks for space consideration.

The major benefit of this approach is the ease with which we can interpret and refine the model, which we do in an iterative process with policy makers and traffic safety experts. The rest of this section presents some highlights from this process. We believe this insight is especially helpful for experts who wish to know what information was and was not used to estimate fatality rates across roads.

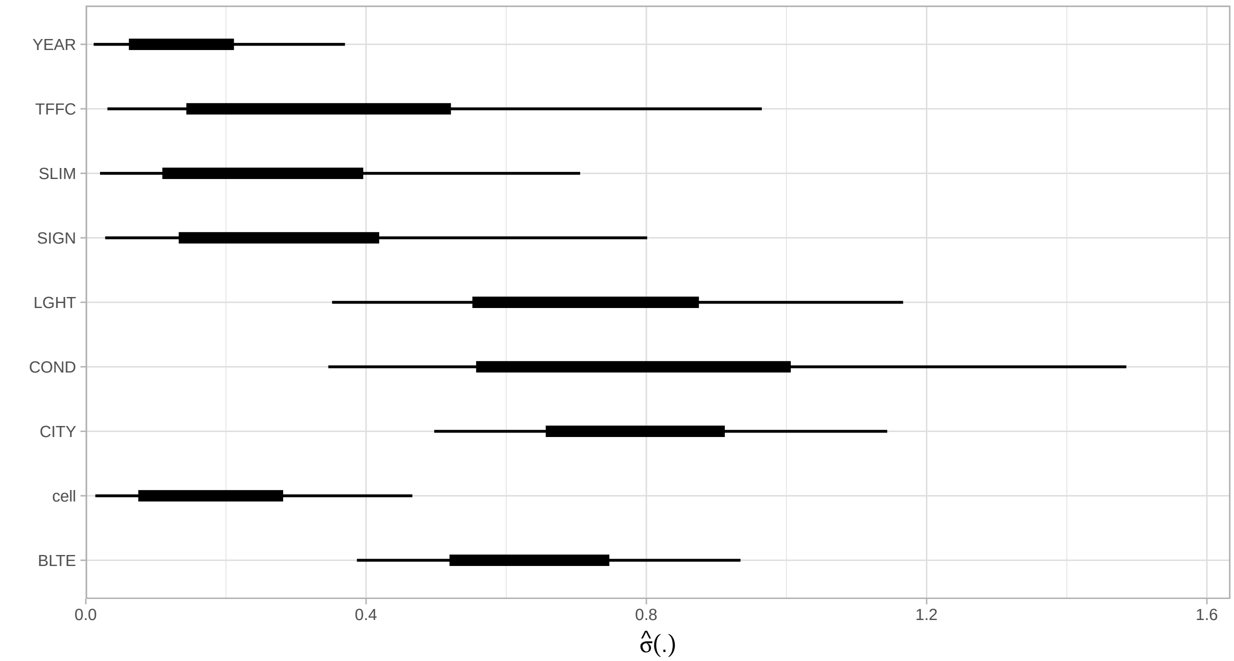
Figures 5 and 6 display an Analysis of Variance using the posterior draws from the first model (Model 1) outlined above. Figure 5 shows the inner 50 and 90 percent quantiles of the finite sample standard deviation. The fat box represents the inner 50 percent range of the parameter, and the thin line represents the inner 90 percent range. Throughout this analysis, we loosely interpret these uncertainty intervals as corresponding to the likely and plausible locations of parameters respectively.

Figure 5 demonstrates that weather and surface condition (COND) and time of day and lighting (LGHT) explain a significant portion of the pedestrian death rate across stratum. This makes sense as these variables capture the varying use of roads each day (i.e. commuting, tourism, dinner, nightlife, etc.). City (CITY) and the built environment (BLTE) also explain much variation in pedestrian deaths. There is relatively little variation of the fatality rate within cells (cell) and years (YEAR) suggesting that, through the other covariates, we have accounted for major sources of variation within strata and between the same strata across years.

TFFC exhibits comparatively erratic behavior. In most draws it explains little variation and is similar to YEAR or the within cell error term. Yet in a few draws, it explains a large amount of variation and is comparable to COND. The reason for this behavior is likely collinearity. Figure 6 shows the 50 and 90 percent quantiles of the mean effects of TFFC. Recall that the model is log-linear so that variation in the mean effects translates to a multiplicative difference in the fatality rate.

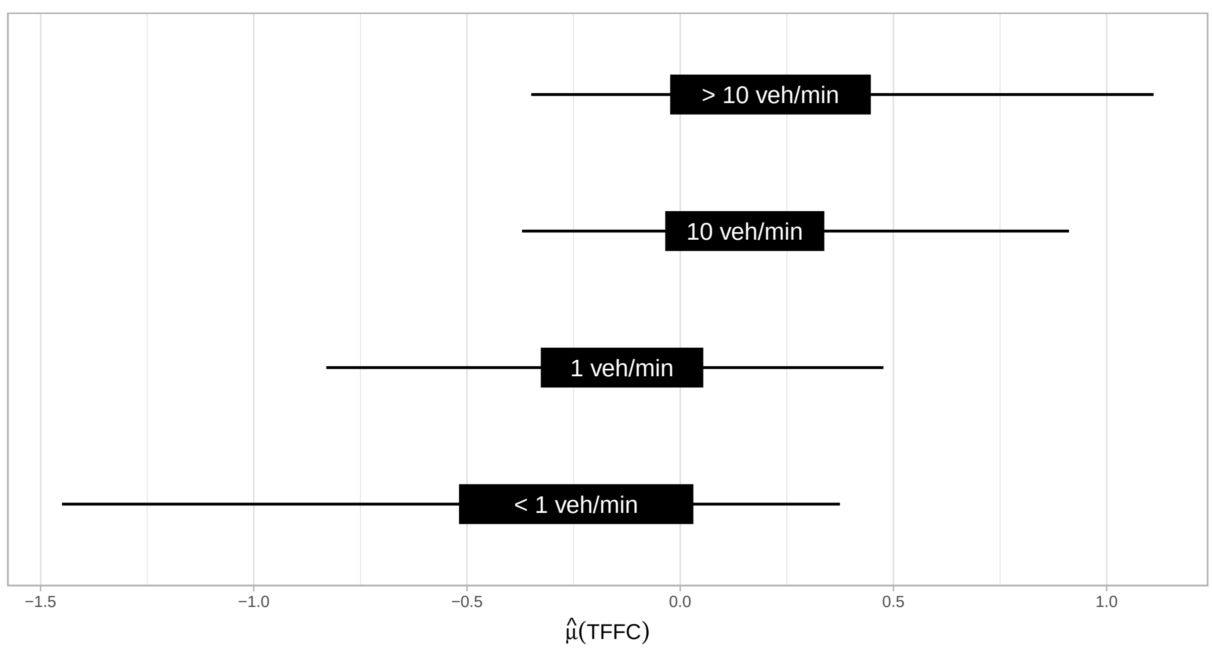
Much of the explained variation is due to whether there is more or less than ten vehicles on the road segment per minute. Aside from this information, there appears to be as much variation within each category as there is between categories. Since the number of vehicles per minute is largely captured by the other variables representing road function, and TFFC has already been determined to be highly associated with CITY, we remove it from future versions of the model. Removing TFFC and not CITY or BLTE makes sense in the context of our introduction: cities build roads and then vehicles drive on them. The variable BLTE, for example, is now not only interpreted as variation due to the physical characteristics of a road but also the subsequent number of vehicles those characteristics support.

Figure 5: Model 1, Analysis of Variance, Between



This figure exhibits inner 50 and 90 percent intervals for the finite−population standard deviations of each batch of explanatory variables. Time of day and lighting, surface condition city and the built environment explain a substantial amount of variation in the fatality rate.

Figure 6: Model 1, Analysis of Variance, Within



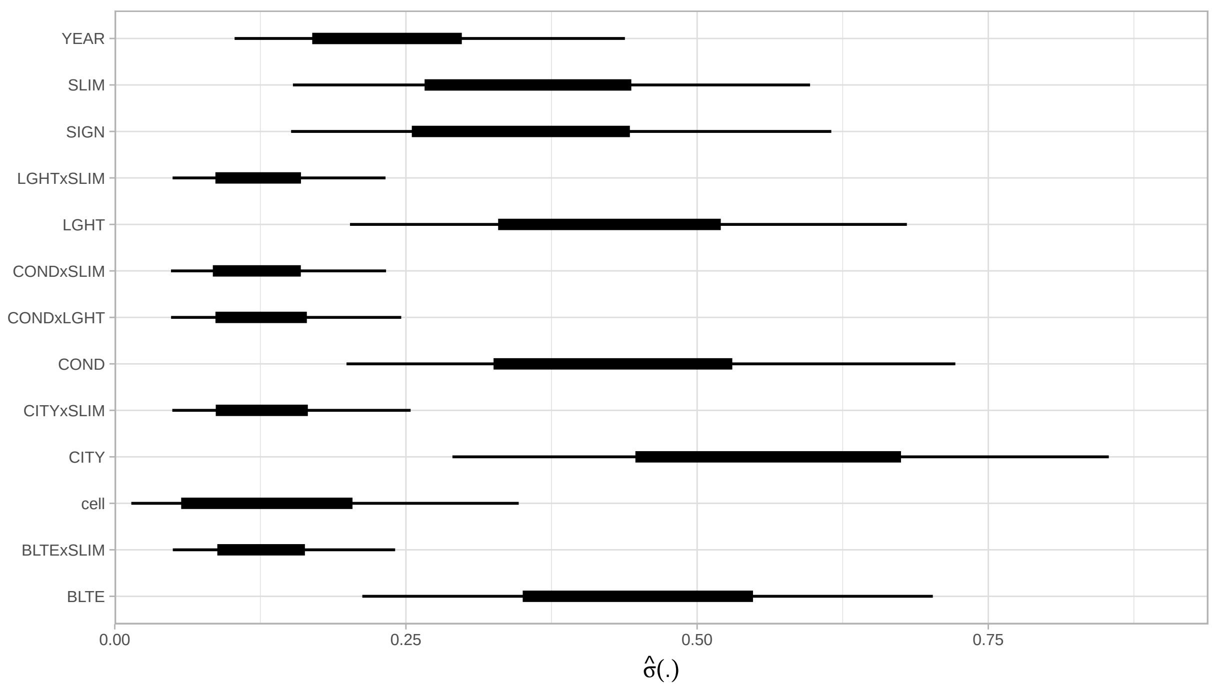
This figure exhibits inner 50 and 90 percent intervals for the traffic effects. Effects are interpreted as the log of the expected multiplicative increase in the fatality rate compared to the average road region. With this model, Traffic effects largely reflect road function within cities and provide little additional information.

Model Expansion with Second Order Interactions

Figure 5 indicates a better model may be obtained by dropping the variable TFFC and adding a qualitative variable, ROUTE, which denotes whether the road segment is a local road, highway, etc. This variable is thought to account for the variation previously explained by TFFC. All $\binom{(}{7}, 2)$ interactions between the covariates, excluding YEAR, were added to the model. However, to deal with the association across interactions, these interactions are given a different set of hierarchical priors. This decision reflects discussions with experts who believed that interactions are likely to explain less variation than marginals and should not be treated as exchangeable. In short, these beliefs were encoded into the prior through the standard deviation parameter of the main effects and the interactions. This resulted in different amounts of regularization, which is visible in the following Figures.

Figure 7 shows select covariates in an updated ANOVA plot using the posterior draws from the posterior distribution of the second model (Model 2). The same computational steps were taken as in the first model of the previous section, and posterior checks are again omitted for space considerations. We note the within cell error term is almost half the size in Figure 5, suggesting that the new covariates jointly explain half the unexplained variation of the original model. As expected from our choice of priors, the marginal variables CITY, LGHT and BLTE all explain less variation after adding the interaction terms. Although, LGHT and BLTE are reduced more by the inclusion of interaction terms than CITY, which now represents the largest source of variation.

Figure 7: Analysis of Variance, Between



This figure exhibits inner 50 and 90 percent intervals for the finite−population standard deviations of each batch of explanatory variables.

Many of the results were consistent with the experience of traffic safety experts. For example, Figure 8.1 shows a few of the LGHT effects, which indicate that the time period of 4-10pm on weekends contains a disproportionately large amount of the road segments with fatalities. Increased risk during dusk has already been identified by researchers, including in the New York City Vision Zero Committee (Taskforce 2017).

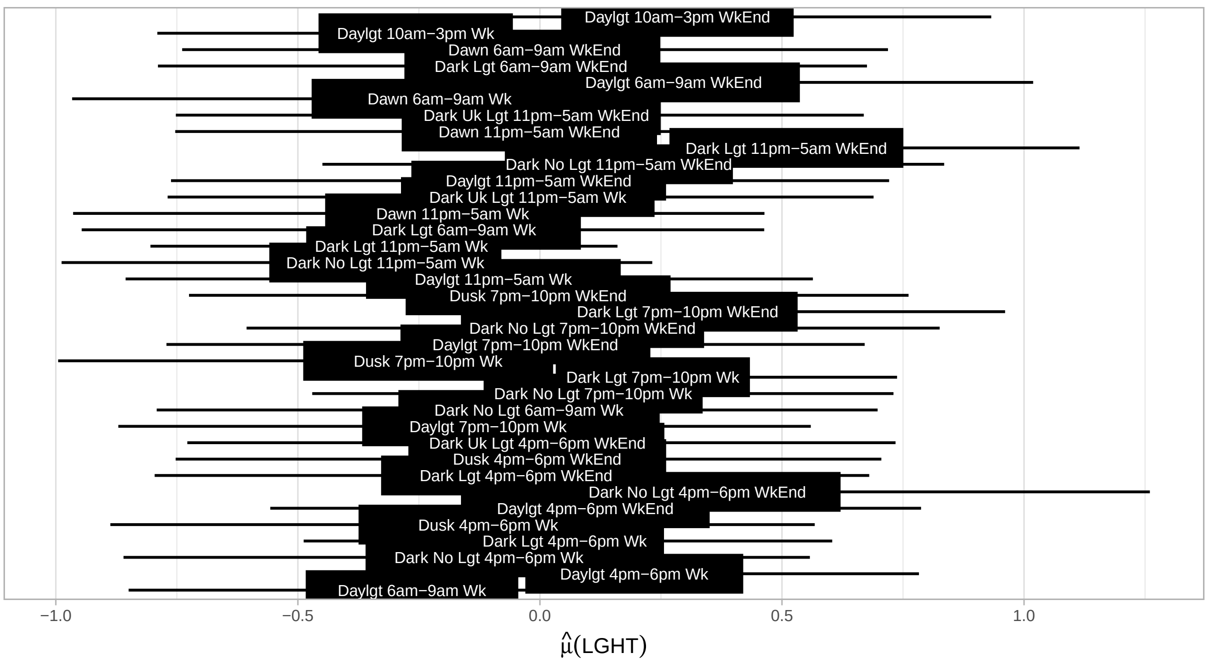
Other results were suspected by experts but had not been quantified and the magnitude of the effects were surprising. For example, Figure 8.2 shows that much of this variation originates from two cities: Los Angeles and New York City. New York City has an average effect of around 1, which is roughly 2.7 times the average city. Conversely, Washington D.C. has an average effect of around -.25, which is roughly 78 percent of the average city. Also Boston had been previously identified as a city with an abnormally large fatality rate. However, Boston does not appear at relatively high risk after adjusting for the other covariates like the weather and built environment. Of course, these effects likely reflect differences in the traffic density that had previously been dropped from the model.

Select interaction effects between CITY and LGHT are also displayed in Figure 8.3, and CITY and COND in

Figure 8.4. There is evidence of variation across these categories. For example, weekend mornings in New York City and cloudy days in San Diego are disproportionately represented in the data. These interactions could reflect city-specific trends, such as the popularity of brunch or the quick advancement of dense fog, but there is too much uncertainty to speculate over the plausibility of these specific explanations.

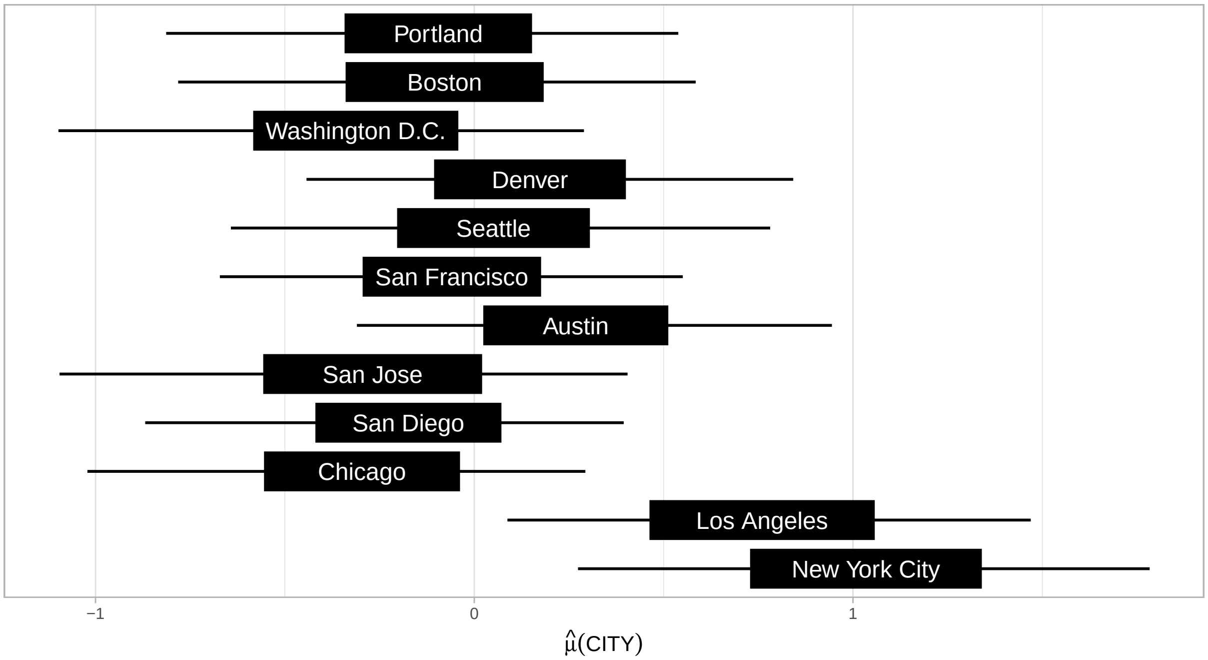
We include these figures to illustrate the variety of inferences and their associated levels of uncertainty that can be surmised from the proposed model. We exclude an even larger number of other plots from our discussion for space considerations. These results, posterior checks and other figures will be available on our GitHub (. . .).

Figure 8.1: Analysis of Variance, Within



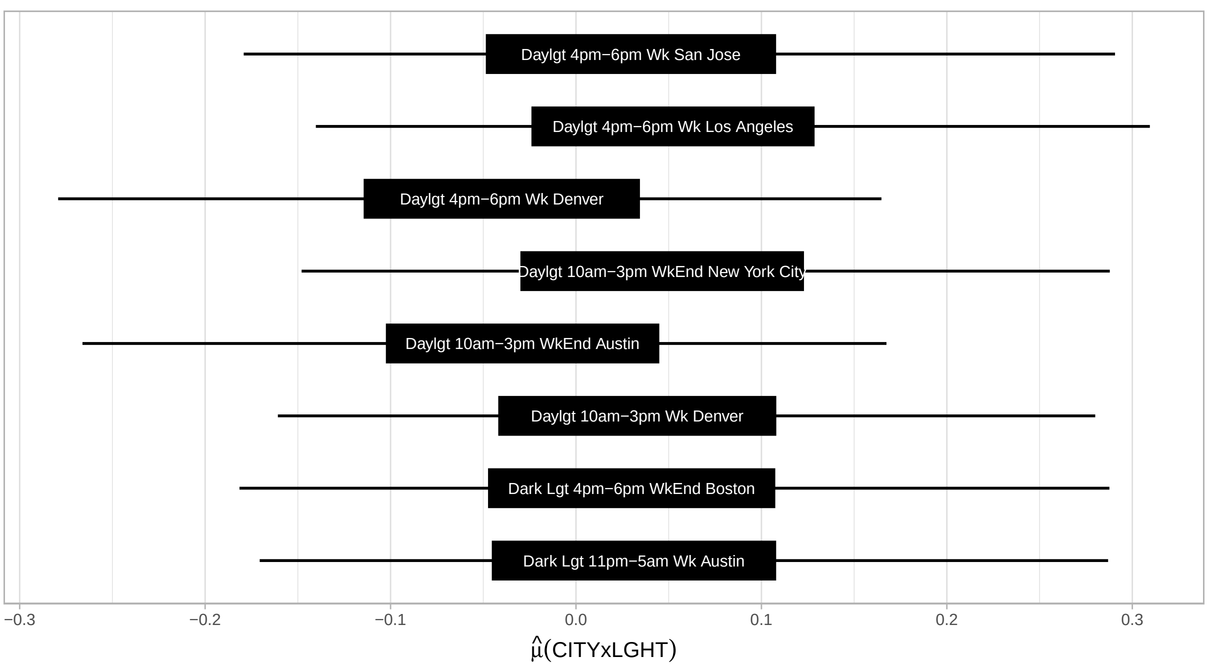
This figure exhibits inner 50 and 90 percent intervals for time of day and lighting effects. Effects are interpreted as the log of the expected multiplicative increase in the fatality rate holding all else constant

Figure 8.2: Analysis of Variance, Within



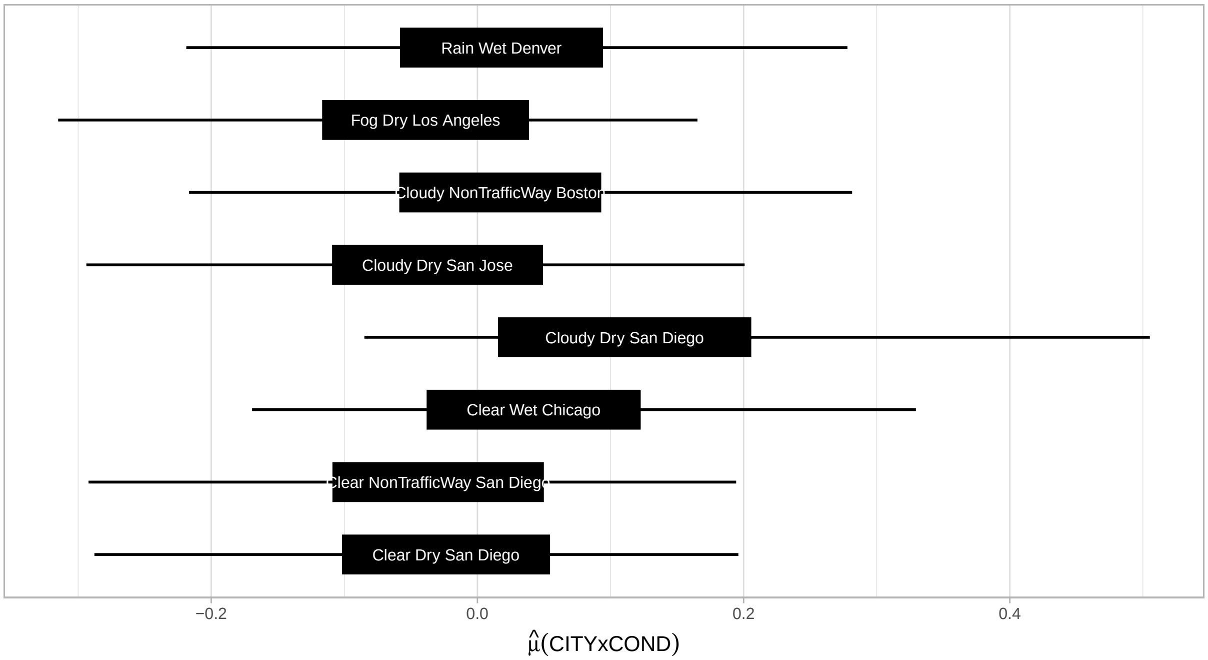
This figure exhibits inner 50 and 90 percent intervals for city effects. Effects are interpreted as the log of the expected multiplicative increase in the fatality rate holding all else constant

Figure 8.3: Analysis of Variance, Within



e exhibits inner 50 and 90 percent intervals for city and time of day and lighting effects. Effects are interpreted as the log of the expected multiplicative increase in the fatality rate holding all else constant.

Figure 8.4: Analysis of Variance, Within



its inner 50 and 90 percent intervals for city and weather and surface condition effects. Effects are interpreted as the log of the expected multiplicative increase in the fatality rate holding all else constant.

IV. Conclusion

We conclude our analysis by resuming our discussion of selection bias in the before-after analyses of New York City's Vision Zero policy. In the Introduction, we presented two competing estimates of the effect of Vision Zero policy at priority locations. A 27 percent reduction was claimed by the Vision Zero committee, and a 27 percent increase from the application of Robbins' formula. We stated the validity of either estimate depended on how well the Vision Zero committee chose the road segments for prioritization. Broadly speaking, if the committee had selected the most dangerous road segments for prioritization, we would expect priority and nonpriority roads to be disparate, and the committee's estimate would more accurately reflect the consequence of Vision Zero policy. Conversely, if the road segments selected by the committee were largely exchangeable, we would expect Robbins' formula to be the more accurate indicator. Both estimates were thought to be extremes, however, with the actual benefit residing somewhere between the two numbers.

We now use samples from the posterior predictive distribution of the final model (Model 2) and the GES sampling weights to calculate the expected number of fatalities on New York City priority and nonpriority road segments each year over the 2010-2013 before period. As explained earlier, year 2009 is excluded from the model due to compatibility issues between GES and FARS. Figure 9 displays the smoothed distributions of the expected number of fatalities for all New York City segment types observed in the FARS dataset. These distributions are not weighted by the number of roads segments represented by each type and reflect the New York City road types on which there was at least one fatality between 2010 and 2013.

We observe a large amount of separation between the covariates predictive of fatalities on priority and nonpriority road segments. This means that priority locations tend to possess covariates that are more dangerous than nonpriority locations and thus have higher fatality rates that that estimated by Robbins' formula in Table 1. This may explain why Robbins' formula suggested an increase in the number of fatalities and not a decrease as suggested by the traffic safety literature. We also point out that the limited overlap between priority and nonpriority road segments makes it difficult to impute counterfactuals and perform a difference-in-difference analysis that compares the fatality reduction on priority roads with the reduction on nonpriority roads.

The total expected and observed number of fatalities on priority and non priority road segments are displayed for Brooklyn, Manhattan and Queens in Figure 11. The expected number of fatalities was caluclated by taking a weighted average of the expected number of each road type represented in FARS, where the weights were the inverse probability of a fatality on each type as determined by the GES sampling weights. We only compute estimates for Brooklyn, Manhattah and Queens, and not Staten Island and the Bronx, because Staten Island and the Bronx are not sampled in the GES. Since these boroughs were eligible to be sampled, we could reweight the results in Figure 11 to reflect all five Boroughs. However, the conclusions would simply be reweighted and the final result would not change significantly.

Black lines in Figure 11 represent inner 50 and 90 percent intervals for the expected annual number of fatalities in the before period. The blue lines represent the average number of observed fatalities from 2009-2013 and the red lines represent the number of observed fatalities in 2016. A regression to the mean effect is apparent in the Figure. For both prioirty and nonpriority road segments, the number of fatalities after Vision Zero has moved towards the average number predicted from the model. However, the blue line for priority Roads has traveled beyond what was expected from the Model, and this movement could be interpreted as reflecting the success of the Vision Zero policy.

There is too much uncertainty to quantify the number of fatalities prevented by Vision Zero to the satisfaction of Vision Zero advocates. The 50 percent uncertainty interval in Figure 11 indicates that there was an average reduction in fatalities between 15 and 31 percent, and the 90 percent uncertainty interval indicates an average reduction anywhere between 1 and 40 percent. At the 95 percent uncertainty level, it is impossible to say whether the policy had no effect or whether it reduced fatalities to the maximal attainable reduction suggested by the literature of 50 percent.

Perhaps more useful is our ability to assign probabilities to statements made by the Vision Zero committee. If we were to evaluate the claim that there was a 27 percent reduction on priority roads in New York City (or for Brooklyn, Manhattan and Queens, a 30 percent decline), our posterior returns a p-value of 25 percent. In other words, we believe after looking at the data that there is a 75 percent chance the reduction is lower than the amount claimed by the Vision Zero committee. Conversely, we find there is a 4 percent chance that the policy led to no change or an increase in fatalities. Thus according to these results, one might believe it is 7.5 times more likely that the Vision Zero committee is right about the magnitude of the benefits than that the policy is not beneficial at all because there was no decrease in fatalities.

In this sense, the Vision Zero committee is perhaps more right in summarizing the benefits of Vision Zero than they are wrong because it has done nothing. However, we find a roughly 50 percent chance the reduction is more or less than 20 percent, three quarters the size of the committee's estimate. We believe this median is a reasonable summary of the effect size of Vision Zero policy, despite the large amount of uncertainty in the average number of fatalities on roads in the before period. It is also close to the 20 percent predicted by Rosén and Sander (2009) and Goodwin et al. (2010) when vehicle speed reductions are enforced commensurately.

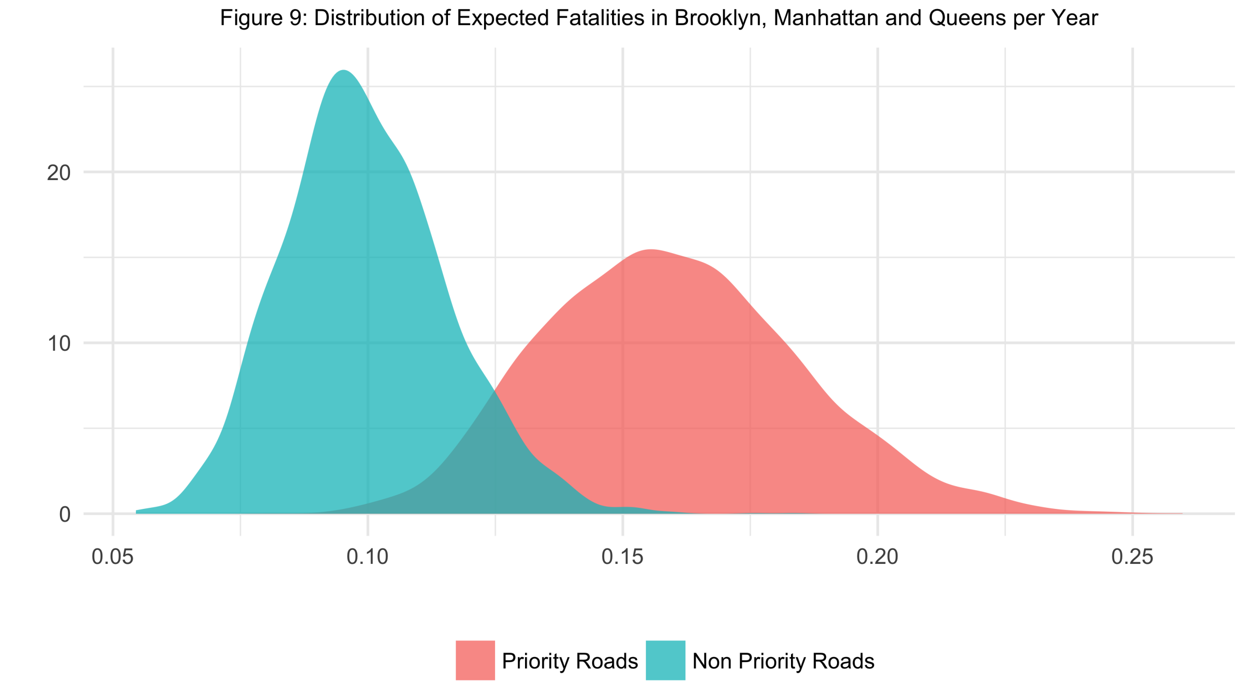


Figure 3: This figure exhibits the variety in the expected number of fatalities in the New York City road segments according to road type and priority status. Priority road segments have been shaded red while nonpriority road segments have been shaded blue.

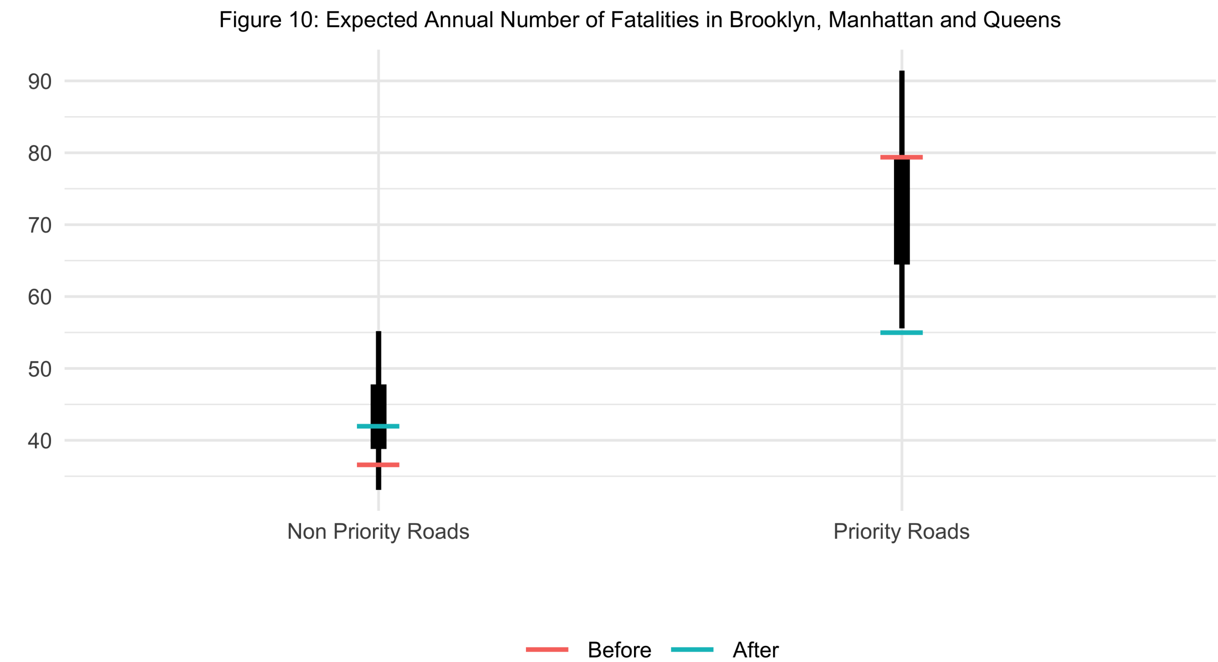


Figure 4: This figure exhibits the expected number of fatalities in the New York City Boroughs of Brooklyn, Manhattan and Queens. The red lines mark the average number of fatalities observed on priority and nonpriority road segments over the 2009-2013 time period. The black lines represent 50 and 95 percent uncertainty intervals of the expected number of fatalities after observing fatalities from 2010-2014. The blue lines mark the observed number of fatalities in 2016.

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